more species of *Limicolæ*, and perhaps of other groups, than I can here name. Is it to be supposed that all these birds, some of them flying by night, make Bermuda by the means which Dr. Weissmann considers sufficient? If his explanation is good it must be good for New Zealand and Bermuda, as well as for the Mediterranean. But there is yet a stronger case to be cited. The Sandwich Islands, as I have learned on authority I cannot doubt (though I know not any mention of the fact in print), are yearly visited like Bermuda, but with even greater punctuality, by large flocks of Golden Plovers—whether *C. virginicus* or *C. fulvus*, is undetermined, but that does not much matter. If the birds belong to the first of these forms they must come from the west coast of America, if to the second from the east coast of Asia. Now there is no land between the Sandwich Islands and the Californian or the British-Columbian coast, but between them and the Aleutians, as I learn from Mr. Rye, there is one islet, Roca de Plata or Crespo. This, however, does not lie in a straight line, and is some 720 miles north-west of the Sandwich Islands, and 1,200 south-west of the Aleutians. Running generally westward of the Sandwich Islands is a series of islets, at distances, perhaps, not exceeding 150 miles, which no doubt might serve as guide-posts for the plovers did they but make for them, but the series comes to an end in about long. 178° W., and though by turning suddenly to the north-east from Morell Island towards Mellish Bank (300 miles), the Aleutians again appear as the nearest land in a northerly direction, the distance of 1,020 miles has to be covered! On the supposition that the birds are of Asiatic birth, and therefore come by another course, we find that due west of Morell Island is Ganges Island, but that is 930 miles off, and thence to reach the easternmost of the Japanese group is 690 miles further! Thus, whichever form of golden plover it be that visits the Sandwich Islands, its regular advent there needs, I think, some fuller explanation than that afforded by Dr. Weissmann's theory.

Then again, there is another set of facts which seem to me irreconcilable with the theory of mere "practice" or "experience." It must be remembered that though Dr. Weissmann relies most on the inherited practice of the *species*, still he does not neglect the *individual*, and both he and Dr. Palmén make considerable use of the observation that adults, and male adults in particular, lead the migratory flocks. This fact, so far as I am aware, has only been noticed in the northward movement in spring, and elsewhere I have endeavoured to account for it ("Encycl. Brit.," ed. 9, iii. p. 767). In autumn it may be doubted whether there is anything of the kind, and we have in many species, the young of the year—birds that are but three months old, or even less, migrating southward with the greatest regularity unaccompanied by adults. This seems to happen with nearly all the *Accipitres*, nearly all the *Limicolæ*, and perhaps, some others, that are bred in arctic or subarctic districts. It happens also with our own Cuckoo (*Cuculus canorus*), and this case is still more wonderful, for the young Cuckoo has had no communication whatever with its progenitors (who have already taken their departure from our shores some weeks earlier), and its foster-parents with us are generally species which do not migrate to any great extent—the Hedge-Sparrow (*Accentor modularis*), Titlark (*Anthus pratensis*), and Pied Wagtail (*Motacilla lugubris*). Yet our young Cuckoos, starting alone and travelling over utterly unknown country, must, on the whole, successfully reach their destination, or the breed would become extinct here.[1] Dr. Weissmann may indeed well say of migrating birds, that the young, when it cracks the shell, possesses "great geographical talent"!

I might easily prolong this article, for there is much more to be said on the subject, and in some details, by no means unimportant, Dr. Weissmann seems to have fallen into errors that I have not here noticed, but my chief object in making these remarks has been to hinder persons who have not previously thought on the matter from taking his easy explanation of the mystery of ornithological mysteries to be sufficient. Believing, as I once before said, that its solution is probably simple in the extreme, and having a strong faith in the hereditary transmission and accumulation of faculties so as to become a wonder-working power, I yet cannot think that he has succeeded in making known the secret and in satisfactorily explaining how birds cross "the sacred spaces of the sea."

ALFRED NEWTON

[1] Since I wrote this I have heard from Mr. Gätke, so well known for his observations on migratory birds, that young Starlings pass over Heligoland during July by hundreds of thousands, "without a single old bird accompanying them," while the old birds begin to migrate at the end of September and continue for the next two months.

THE DIMENSIONAL PROPERTIES OF MATTER IN THE GASEOUS STATE

By assuming a sufficient number of sufficiently insignificant individuals to constitute a group, it is possible to imagine a state of things in which as far as it can be observed from a certain distance, all evidence of individual action is entirely lost. In this way has been framed the molecular hypothesis or kinetic theory of gas. But it must be obvious to every one who has considered this molecular hypothesis, that the apparent uniformity in the actions we perceive must be the result of the distance (so to speak) from which these actions are observed, and that could we sufficiently localise (as regards time and space) our observation, we must perceive in all their varieties the individual actions of the molecules. And even failing this, between the actions of the individuals and the absolute mean action there must be local or parochial actions which would be distinguishable at greater distances than the purely individual actions.

In order that the properties of a gas may appear perfectly uniform in all directions and quite independent of the shape and volume of the space which the gas is constrained to occupy, the number of molecules must be countless, and the temporary action of each individual must be confined to an imperceptibly small portion of the space observed. If these conditions are not fulfilled then the properties of the gas will not be uniform, and we must have dimensional properties depending on the dimensions of the constraining surfaces.

The idea of our being able actually to perceive such properties does not appear to have been entertained hitherto. Until fifty years ago, all the mechanical properties of gases were regarded as quite uniform, the only mechanical distinction between one kind of gas and another being that of weight. Since that time in the phenomena of diffusion, and the phenomena attending the passage of gases through minute channels, properties of gases have been recognised which readily distinguish between one kind of gas and another, and even more than this, for Graham found that there was a difference in the relative behaviour of different gases in differing through porous plates of different coarseness. Still, neither Graham nor any one else appears to have recognised evidence of dimensional properties.

Neither did the development of the mathematical theory lead to the revelation of dimensional properties. Since 1864 it has been known that this theory included the explanation of all the uniform properties of gas. But in developing this theory attention appears to have been paid exclusively to the mean of the motions. And although Prof. Maxwell points out that there must ultimately be dimensional properties, he has not pursued the investigation, so as to reveal their character.

In 1874 a very remarkable phenomenon was brought to light by the experiments of Mr. Crookes—that in ex-

tremely rare media light bodies are apparently repelled by hot and attracted by cold bodies. At first this was not recognised as a gaseous phenomena—in fact the non-presence of gas was supposed to be essential to the occurrence of the highest form of the action.

That such motions might result from the inequalities in the pressure of the residual gas caused by the communication of heat between the objects and the gas, was first shown by the author in May, 1874 (*Proc.* Roy. Soc. 1874, p. 402).

It was shown that when heat is passing from a surface to a gas whether by direct communication or by evaporation the reaction from the receding molecules causes an excess of pressure proportional to the heat communicated.

The reasoning was definite so far as it went and, although manifestly incomplete, the conclusion arrived at, viz., that the phenomena resulted from the heat communicated to the residual gas appears very soon to have been generally accepted as it was found to be verified in various ways. Several papers appeared in which attempts were made to render the explanation more complete, but these attempts were for the most part based on a misconception of the phenomena to be explained and were altogether wide of the mark.

In November of 1878 the clue to the step that was wanting to complete the explanation occurred to the author. It was then seen that although the surfaces with which the phenomena occurred were of limited extent, no account had been taken of this fact in the attempts to explain the actions. Having once hit on this point the deduction of a complete theory of the phenomena was only a matter of labour. It was found that although the excess of pressure is proportional to the quantity of heat communicated, it also contains a factor which is proportional to the divergence of the lines of heat flow; and hence the reciprocal of the density of the gas at which the phenomenon would occur should vary inversely as the size of the vanes. So that by using vanes of comparatively small size the phenomena should be obtained at proportionally greater densities of the gas.

On considering how this conclusion might be experimentally tested it appeared that in order to obtain any results at measurable pressures the vanes would have to be very small indeed, too small almost to admit of experiment. It was while searching for some means to overcome this difficulty that it became apparent that if the vanes were fixed, then, instead of the movement of the vanes, we should have the gas moving past the vanes —a sort of inverse phenomenon—and then instead of small vanes small spaces might be allowed for the gas to pass. Thus the probable existence of phenomena of *Thermal Transpiration* was suggested, and it was obvious that the porous plug would furnish the means of verifying the conclusions. This probable connection between the phenomena of the radiometer, which may be called *Impulsion*, and the phenomena of transpiration through porous plugs raised the question whether the same extension of the dynamical theory of gas which explained the radiometer would not include the results obtained by Graham not then explained. This idea was followed up, and a method was devised of extending the dynamical theory of gases so as to take into account the forces tangential, and normal, arising from a varying condition of molecular gas.

This theory appeared to explain fully all the results established by Graham as well as the then known phenomena of impulsion (the radiometer), besides definitely indicating the phenomena of thermal transpiration to be expected, as well as the effect of employing small vanes in the radiometer. That this step had been accomplished was intimated by the author in the following passage in a letter published in NATURE, vol. xix. p. 220.

"Now, however, I have arrived at a result, which, although somewhat unexpected and striking, will, I hope, be found to reconcile what has hitherto appeared to be anomalous in the phenomena already known, and to have suggested certain hitherto unexpected phenomena which now only await experimental verification."

This experimental investigation which was at once commenced, proved to be a much more serious undertaking than had been anticipated, and was not completed until the end of August, this was not so much on account of difficulties, although these were considerable, but because it was found possible to do so much more than had been expected.

One of the results of the investigation was to show that a difference of pressure is maintained whenever two chambers of the same gas are separated by a porous plate, one face of which is hotter than the other.

With a plate of meerschaum ·44 inch thick, one side of which was something less than 212°, while the other side was about 50°, the difference of pressure was ·25 inch of mercury with air at the atmospheric pressure, and ·88 of an inch with hydrogen at the same pressure.

The existence of this thermal transpiration, although a new phenomenon, is not considered to be the most important result of this part of the investigation, for it appeared on comparing the results obtained with different plates, and different densities of gas, that there was a constant relation between the pressures for different plates at which proportional results were obtained. Thus comparing a plate of stucco with a plate of meerschaum, it was found that the ratios of the difference of pressure to the mean pressure, was the same for both plates so long as the mean pressure with the meerschaum was six times greater than with the stucco, and that this law held although the mean pressures with the meerschaum ranged from 30 to 1·25 inches of mercury. This ratio of the pressures at which corresponding results were obtained, was the same whether the gas used were hydrogen or air; and for plates of different thickness, and hence it was clearly shown to depend only on the coarseness of the plates.

Thus in thermal transpiration we have a phenomenon of gaseous motion depending on a relation between the density of the gas and the dimensions of the space which it is constrained to occupy. The discovery of this relation between the density of the gases and the coarseness of the plates at which corresponding results of thermal transpiration were obtained, suggested the possibility of obtaining a like relation for corresponding results when the gases were forced through the plates by a difference of pressure. Graham had found that the relative rates at which different gases transpired through plates differed very considerably with the coarseness of the plates and no explanation had been given of the phenomena. On trying the experiments not only was it found that with plates of different coarseness corresponding results were obtained whenever the pressures of the gas have a constant ratio but it was also found that with the same plates the ratio was the same as in the case of thermal transpiration.

A successful attempt was then made to verify the conclusion that the phenomena of the radiometer might be obtained at higher densities by using smaller vanes. By suspending fibres of silk and spider lines the repulsive action of heat was rendered apparent at pressures ranging up to the pressure of the atmosphere.

These results, as well as the theory from which they were deduced, have been fully described in a paper, an abstract of which was read before the Royal Society on the 6th inst.

As regards transpiration and impulsion, the investigation appears to be complete; most, if not all, of the phenomena previously known have been shown to be such as must result from the tangential and normal stresses consequent on a varying condition of a molecularly constituted gas; while the previously unsuspected

phenomena to which it was found that a variation in the condition of gas must give rise, have been found to exist.

The results of the investigation lead to certain general conclusions which lie outside the immediate object for which it was undertaken; the most important of these is that gas is not a continuous plenum.

The experimental results considered by themselves bring to light the dependence of a class of phenomena on the relations between the density of the gas and the dimensions of the objects, owing to the presence of which the phenomena occur. As long as the density of the gas is inversely proportional to the coarseness of the plates, the transpiration results correspond; and in the same way, although not so fully investigated, corresponding phenomena of impulsion are obtained as long as the density of the gas is inversely proportional to the linear size of the objects exposed to its action; in fact, the same correspondence is found with all the phenomena investigated.

We may examine this result in various ways, but in whichever way we look at it, it can have but one meaning. If in a gas we had to do with a continuous plenum, such that any portion must possess the same properties as the whole, we should only find the same properties, however small might be the quantity of gas operated upon. Hence, in the fact that we find properties of a gas depending on the size of the space in which it is inclosed, and on the quantity of gas inclosed in this space, we have proof that gas is not continuous, or, in other words, that gas possesses a dimensional structure.

In virtue of their depending on this dimensional structure, and having afforded a proof thereof, it is proposed to call the general properties of a gas on which the phenomena of transpiration and impulsion depend, the *Dimensional Properties of Gas.*

Although the results of the dimensional properties of gas are so minute that it has required our utmost powers to detect them, it does not follow that the actions which they reveal are of philosophical importance only; the actions only become considerable within extremely small spaces, but then the work of construction in the animal and vegetable worlds, and the work of destruction in the mineral world, are carried on within such spaces. The varying action of the sun must be to cause alternate inspiration and expiration, promoting continual change of air within the interstices of the soil as well as within the tissue of plants. What may be the effect of such changes we do not know, but the changes go on; and we may fairly assume that, in the processes of nature, the dimensional properties of gases play no unimportant part.

OSBORNE REYNOLDS

OUR ASTRONOMICAL COLUMN

THE BINARY STAR α CENTAURI.—Dr. Doberck, with the aid of measures made during the last few years has calculated elements of α Centauri, which, though given as only provisional, will doubtless approach nearer to the true ones than any previously published. They are as follows:—

Passage of the peri-astre	1875·12
Node	25° 32′
Angle between node and peri-astre	45° 58′
Inclination	79° 24′
Excentricity	0·5332
Semi-axis major	18″·45
Period of revolution	88·536 years.

Comparing this orbit with weighted means derived from Mr. Gill's measures at Ascension in 1877, the following differences are shown:—

1877·614	...	Position + 2°·32	...	Distance − 0″·23
1877·858	...	,, − 0·39	...	,, + 0·05

For 1879·5 the elements give position, 173°·4; distance, 3″·47; and for 1880·5, position, 185°·2; distance, 5″·30; the smaller star will be due south of the larger one at the beginning of 1880, distant 4″·37. The above value for the semi-axis major, taking the annual parallax of α Centauri, a mean between the values of Maclear and Moesta, indicates that the mean distance between the component stars is rather greater that the mean distance of Uranus from the sun. Frequent measures of α Centauri during the next few years are much to be desired.

A NEW VARIABLE STAR IN SAGITTARIUS.—The following case appears a singular one, if the star is not variable to a considerable extent:—On April 28, 1783, D'Agelet observed a star which he estimated of 4·5m., and which is No. 4,627 in Gould's Catalogue. It does not occur in Piazzi or Lalande, nor in Bode, but it is found on Harding's Atlas as 6m. It is wanting in the Uranometries of Argelander and Heis, but the former observed it three times in his Zones. In Z. 218, July 2, 1849, it is rated 5m.; in Z. 225, on July 13, only 7m.; and in Z. 391, June 30, 1851, it is 5·6m. The mean position for 1850 from Argelander's observations is in R.A. 17h. 59m. 6·57s., N.P.D. 107° 10′ 9″·9, or reducing to 1880 in R.A. 18h. 0m. 51·1s., N.P.D. 107° 10′ 10″. It is difficult to account for such an object having escaped the notice of other observers, except upon the supposition of variability; perhaps, like some other variables, it is only conspicuous for a short time. The star follows 6 Sagittarii 6m. 26s., in 1′·4 greater N.P.D. It is proper to state here that Mr. J. E. Gore, in his "Southern Stellar Objects," p. 104, has a reference to this star amongst stars possibly variable, but the observations of D'Agelet and Argelander do not appear to have been known to him.

NEW MINOR PLANET.—No. 193 was discovered by M. Coggia at Marseilles on March 1, not far from the place of No. 192, detected at Pola by Herr Palisa, in the previous month.

INTRA-MERCURIAL BODIES.—In a letter addressed to M. Mouchez, communicated to the Academy of Sciences at Paris on March 3, Padre Ferrari, Director of the observatory of the Collegio Romano, mentions that, having had occasion to institute researches respecting the observation of a rapidly-moving spot upon the sun's disk by De Cuppis on October 2, 1839, at the instance of Prof. Oppölzer, he had met with particulars of a similar observation by De Vico in 1837. Reference is made to this observation in "Memoria intorno ad alcune osservazioni fatte alla Specola del Collegio Romano, 1838," p. 15, but the year only is there mentioned. De Cuppis, a friend and frequently co-operator of De Vico's has, however, preserved the date in the journal *L'Album* for 1838, July 7, where the observation is thus described: "In una osservazione del 12 luglio, 1837 parve al sullodato astronomo (De Vico) del Collegio romano veder rinnovato il fenomeno, in una piccolissima macchia perfettamenta rotonda e senza traccia della cosi detta penombra, la quale nel breve spazio di 6 ore trascorse buona parte del disco solare."

This observation does not occur in Haase's Collection, nor is there here more than a reference to the observation of Decuppis, which is thus given in a note by Arago at the sitting of the French Academy on December 16, 1839:—"M. Decuppis annonce que le 2 Octobre, en continuant des observations qu'il faisait sur les taches du soleil, il a vu une tache noire, parfaitement ronde et à contours nettement terminés, qui s'avançait sur le disque de l'astre d'un mouvement propre rapide, de manière à ce qu'elle a dû en traverser le diamètre dans environ six heures. M. Decuppis pense que les apparences qu'il a observées ne peuvent s'expliquer qu'en admettant l'existence d'une nouvelle planète."

There are other observations upon record in January